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Transfers and the effect of monitoring in an overlapping-generations experiment

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Abstract

We experimentally investigate the argument that the establishment and maintenance of an intergenerational transfer system is related to cross-generational monitoring. In our overlapping-generations experiment transfers are induced to be collectively efficient, but individually suboptimal. Varying the information conditions allows us to study the relationship between the level and development of voluntary transfers, on the one hand, and the possibility for cross-generational monitoring and rewarding and punishing, on the other hand. The experimental results give little support for the importance of monitoring. Nevertheless, a fairly efficient level of transfers is observed. The results suggest that the public good feature of the setting is more important than the bargaining feature, which would require intertemporal rewards and punishments. © 1998 Elsevier Science B.V. All rights reserved.

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1. Introduction

Despite the growing financial strain caused by the ageing of the population, pay-as-you-go public pension systems enjoy wide public support. This raises the question of the determining factors of these transfer systems. It is often argued that one crucial factor is the perception by current generations of the support the system will receive from future generations. There is also no guarantee that today's decisions will not be overturned tomorrow. This temporal credibility problem implies that decisions taken today must, in some way, be related to decisions taken in the future. In some sense, the system must be self-enforcing. In this paper, the presence and source of such self-enforcing mechanisms are studied in an experimental environment.

One mechanism that could explain a stable public pension system is the presence of a voluntary 'social contract' between successive generations. Even if generations are not altruistic towards other generations, (implicit) social contracts with positive intergenerational transfers can be supported as a Nash-equilibrium. [1] Ingredients of such a contract are, first, the obligation to provide the elderly with a transfer equal to some prescribed level if they have adhered to the contract themselves, and, second, a punishment rule if the elderly broke the social contract. One problem with this approach is that there often are many alternative social contracts which could lead to a stable pension system. Which of these contracts will occur depends upon the expectations held by successive generations and the way they solve any coordination problems.

A less sophisticated but related explanation is that successive generations 'build up confidence' in the maintenance of the system by looking at the past performance of the system. It can be shown that public transfers can converge to socially efficient levels, if the confidence in the system grows when successive generations keep following the scheme. [2]

Under both the social contract approach and the confidence-building approach, the relation between past and present decisions plays a central role. By monitoring the behaviour of past decision makers, current decision makers decide whether to support a transfer system. In reality, preferences and decisions are aggregated in a complex (political) process. Therefore, in empirical data on the development of pension schemes, disentangling any of the above mentioned factors will be difficult, if not impossible.

As in many areas of economics, a more detailed inspection of the determining factors of decision-making is possible (only) in a controlled experimental environment. In the present paper we study individual decisions about transfers in an experimental overlapping-generations (OLG) setting. We examine the develop-

[1] See Sjoblom (1985) and Kotlikoff et al. (1988) for a related model. See Veall (1986), for a model that demonstrates that altruism in itself is not sufficient to engender intergenerational transfers.

[2] See Verbon (1987) and Van Dalen and Van Praag (1992) for this approach.

ment of transfers in a setting where player (generation) P_t decides on a transfer to player P_{t-1}. In turn, player P_{t+1} decides on a transfer to player P_t; then player P_{t+2} decides on a transfer to player P_{t+1}, and so on. A voluntary transfer system is induced to be collectively efficient. Intergenerational transfers allow for the smoothing of consumption over time. On the other hand, transfers are individually irrational because the direct private benefits of giving a transfer are negative.

The central question of our investigation is whether allowing the present generation to monitor and to react on the transfers of the previous generation(s) facilitates the development and stability of a voluntary system of transfers. To that purpose we employ two information treatments. In one treatment, player-generations are supplied with information about the transfer levels of previous generations. In the second (control) treatment they are not supplied with this information. So, if monitoring and the possibility of rewards and punishments across generations adds to the development and stability of a voluntary transfer system, then this should show up as a difference between the two treatments.

Our study is related to experiments on the voluntary provision of public goods. Also in the public-goods experiments there is a tension between collective and individual rationality. The typical finding there is that contributions are clearly bounded away from the individually rational level of zero, but fall short of the collectively efficient contribution level (see Ledyard, 1995, for a survey of the literature). Public-good experiments, however, lack the intertemporal structure that is characteristic of decision making on intergenerational transfers.

Our study is also related to experiments on the role of altruism and reciprocity in bilateral bargaining. In gift exchange experiments, for example, the responder's return on the proposer's gift is often found to be increasing in the size of the gift (Fehr et al., 1993; Berg et al., 1995). Similarly, in ultimatum bargaining experiments the probability that a proposal is accepted by a responder is increasing in the share of the cake the proposer is prepared to give to the responder (Güth and Tietz, 1990). So, reciprocity can be observed in a bilateral relationship if both sides have some power. Experimental evidence suggests, furthermore, that the latter condition is not only sufficient but also necessary for gift giving to occur. If the receiving side of the relationship has no power at all, the proposer's gift decreases drastically. For example, in Forsythe et al. (1994), the modal proposal is about 50% of the \$5 cake to a receiving player with veto power but less than 10% to a receiver with no reciprocal power (see also Güth and Van Damme, 1994). The experimental games studied in the present paper are, in some sense, in between the dictator games with no reciprocal power and the ultimatum or gift exchange games with full reciprocal power. Rewards and punishments may be anticipated by the sender of a gift, but they (if at all) are not supplied by the receiver of the gift but by a third party, namely the next generation-player.

The paper proceeds as follows. The next section presents the underlying model and the two main questions of the experiment. Section 3 describes the experimental design. Results are presented in Section 4. Because we wanted to examine the robustness of the results, we have run two additional series of experiments. Section 5 briefly discusses the results of these additional sessions. Finally, Section 6 contains a concluding discussion.

2. An OLG model with transfers

The model that forms the basis for our experiments is a simple two overlapping-generations (OLG) model in which each generation consists of one player. Each player lives for two periods. In the first period (when young), a player is endowed with a transferable endowment of 7 and a non-transferable endowment of 2. In the second period (when old), a player only receives a non-transferable endowment of 1. The young player in period t decides about the part T_t of the transferable endowment of 7 he wishes to transfer to the current old player, $0 \leq T_t \leq 7$. The remaining endowment is used for 'consumption'. So, first period consumption C_{1t} of player P_t (when young) is given by:

$$C_{1t} = 9 - T_t. \tag{1}$$

Second period consumption C_{2t} of player P_t (when old) is given by:

$$C_{2t} = 1 + T_{t+1} \tag{2}$$

where T_{t+1} is the transfer player P_t receives from player P_{t+1} in the second period of his life. Total utility U_t of player P_t, is given by the following utility or pay-off function ($t \geq 1$): [3]

$$U_t = C_{1t} \times C_{2t} = (9 - T_t)(1 + T_{t+1}). \tag{3}$$

The form of the pay-off function reflects the fact that consumption in both the first (young) and the second (old) period of one's life matters. The multiplicative form, in addition, implies that it is optimal to smooth consumption over both periods.

First consider the decision problem of a (non-altruistic) player P_t. This player faces the following problem:

$$\max_{0 \leq T_t \leq 7} (9 - T_t)(1 + T^e_{t+1}) \tag{4}$$

where T^e_{t+1} is player P_t's expectation about the next player's transfer. It is easily seen that if P_t expects T_{t+1} to be unrelated to T_t, he will choose to transfer $T_t = 0$.

[3] Of course, in the experiment the sequence of players has to be started and stopped. The first player in the sequence, P_0, only plays the role of the receiving (old) player. The final player in the sequence, P_7 in the experiments, only plays the role of the transferring (young) player. No experimental standard has been developed yet on how to deal with this. In the experiment we chose to set player P_0's first period consumption (when young) equal to the basic endowment: $C_{10} = 2$. Player P_7's received transfer (when old) was set equal to the average transfer to all previous receivers (rounded up). To a large extent this starting and stopping rule is an arbitrary matter. To check whether our results are affected by this choice, we ran an additional design (discussed in Section 5) which mitigates the impact of this starting and stopping rule.

If all players choose a transfer of 0, then the pay-off to each player will be 9 (and 2 for P_0; see footnote 3). In a finite sequence of players (as we will have in our experiment) the unique Nash-equilibrium is for all players to transfer zero.

The players forego considerable pay-off opportunities when all choose a transfer of zero. From Eq. (3) it follows that if all players choose a transfer of 4, they would all receive a pay-off of 25 (10 for player P_0). The question is: How could such a transfer come about? It is easily seen that a necessary condition for any positive transfers to be given by a non-altruistic player is that some relation exists between present and (expected) future transfers. For example, if strict 'following behaviour' is anticipated by P_t, that is, $T^e_{t+1} = T_t$, then the transfer that maximizes P_t's expected pay-offs can be calculated to be equal to $T_t = 4$. [4] Obviously, player P_t cannot control the reaction T_{t+1} of player P_{t+1} to his own transfer T_t. It depends on the future players' willingness to cooperate whether positive transfers can be established.

Non-altruistic players will only be willing to give positive transfers if they have some confidence that positive transfers will be rewarded by the next player. In other words, the players should expect some relationship to exist between past, current, and future transfers. A specification in line with this idea, suggested by Van Dalen and Van Praag (1992), is the following:

$$T^e_{t+1} = T_t + \sigma(T_t - T_{t-1}), \quad \sigma > -1, \tag{5}$$

where σ denotes the degree of confidence or the support expected by the next generation. The current generation is expecting to be rewarded for its own transfer, and, in addition, it expects to receive a premium for increasing the transfer level above the level provided by the previous generation. [5]

Another, more strategic approach which could explain positive transfers is the social contract model, mentioned in the introduction. In an infinite sequence of overlapping generations, positive transfers can be supported as a Nash equilibrium. In the experiment we will have a finite sequence, however. Nevertheless, in finitely repeated games, experimental subjects are sometimes seen (to learn) to employ 'trigger-like' strategies to support outcomes that are non-Nash in the stage game (see, e.g., Selten and Stoecker, 1986; Camerer and Weigelt, 1988). Similarly, if such trigger strategies are (learned to be) employed and anticipated in our finite OLG game, they might lead to positive transfer levels. Central to such strategies is that generations are punished for deviating from the implicitly agreed

[4] It can be calculated that (due to our starting and stopping rule) $T = 4\frac{1}{7}$ is the stationary level of transfers that maximizes joint pay-offs of all players, including P_0. In the experiment we restrict transfers to integer values. Hence, also from this respect, $T = 4$ seems to be a useful benchmark.

[5] A problem with this specification is that it is not consistent with expected pay-off maximization. It is easily checked that maximization of Eq. (4) subject to Eq. (5) leads to a solution for T_t which is usually inconsistent with the hypothesized relation in Eq. (5). In other words, if players' expectations about the transfers of others are in accordance with Eq. (5), their own transfers will not be in accordance with Eq. (5) under expected pay-off maximization.

upon transfer level, unless this deviation was in order to punish a previous deviating generation. In other words, deviators and non-punishers are punished, but punishers are not. Clearly, if such a strategy is anticipated, then following the implicit contract would be optimal (for all but the last generation).

The central question now is whether players are actually willing and able to sustain positive transfer levels in this setting. Of course, this will depend on the motivations and expectations of the players. In this paper we examine, in particular, whether the possibility of monitoring previous generations facilitates the realization of collectively beneficial positive transfers levels. To that purpose we employ two information treatments: treatment I (information) and treatment N (no information). In treatment I, each player generation P_t is provided with information about the transfers of previous generations $(T_1, \ldots, T_{t-1})$ in the sequence. In (control) treatment N, players are not provided with this information. This allows us to test the theoretical supposition that the support for collective (transfer) schemes can be explained by an implicit social contract, the successive build up of confidence, or any other norm or rule which requires cross-generational monitoring. The main two questions of our inquiry are then:

Q1. Does the possibility of monitoring the transfer levels of previous generations facilitate the occurrence of positive transfers?

Q2. Do we observe a systematic relation between the transfer level of the present generation and that of the next generation(s) in information treatment I?

The two questions are, of course, interrelated. An affirmative answer to question Q1 requires an affirmative answer to question Q2 almost by necessity. If monitoring facilitates the occurrence of positive transfers, then this facilitating role should come about through some (positive) relation between present and future transfers.

The reverse, however, need not necessarily hold. If we were to find a systematic positive relation between present and future transfers, then the average level of transfers might still be lower in treatment I than in treatment N. The reason for this is that with a positive relation between T_t and T_{t+1} low transfers in period t will be followed by low transfers in period $t+1$ (and $t+2$, $t+3$, etc.). So, if in the initial period or in some later period a low level of transfers is observed, this might lead to a low level of transfers after that period. Such a chain of low transfers owing to punishments can occur in information treatment I but not in information treatment N. However, if such an outcome were to be observed – that is, an affirmative answer to Q2 and a negative answer to Q1 – we would at least expect the (low) level of transfers in treatment I to be more 'stable' than in treatment N. Therefore, in discussing the results, we will not only compare the average levels of transfers across the two treatments, but also the variability of the transfers.

Three final remarks concerning our setup must be made. The first remark concerns the motivation of the experimental subjects. We are interested to see whether individuals achieve higher levels of cooperation when there is a possibility to monitor the adherence to certain standards or rules of conduct. However, in the game-theoretical benchmark (with zero transfers) the players are assumed to be non-altruistic income maximizers, not inhibited by any social norms or rules. ''Note, however, that to design an experiment that allows one to contrast a more complex theory with a theory based on simple income maximization, it is nevertheless necessary to know what the income maximization theory predicts, so that it is necessary to control for the predictions of the simpler theory even when more complex theories of behaviour are being examined'' (Roth, 1995a, p. 80). Therefore, we follow conventional conduct and try to induce such purely non-altruistic individual motivations on the experimental subjects.

Nevertheless, as is well recognized ''experiments suggest that bargainers may be concerned with more than their own pay-offs in evaluating outcomes. On a purely methodological level, this illustrates how difficult it is to gain complete control over the experimental environment'' (Roth, 1995b, p. 328; see also Ledyard, 1995, p. 169). Hence, if experimental data are not in line with theory it is possible that the preferences assumed in the theory do not correspond to those of the experimental subjects.This issue of (internal) validity is most often raised if the experimental data do not correspond to the predictions of a theory. [6] Therefore, we do not simply compare the experimental results to theoretical predictions, but base our main conclusions on a comparison of the two information treatments. ''[T]he strongest conclusions from an experimental study come from *within experiment* comparisons, which report the effect of a change of a single variable, while holding others constant'' (Roth, 1988, p. 1023, emphasis in the original).

Second, it seems important to allow the subjects to learn and understand the structure of the OLG game. In our basic design we choose to have several repetitions (15) of an OLG game with a restricted sequence of generations (8) and no 'reincarnation' (like Cadsby and Frank, 1990), rather than one OLG game with a long sequence of (say, 120) generations and reincarnation (like Marimon and Sunder, 1993). Consequently, the backward-induction argument of unravelling might apply to our design. The last generation in each OLG game might learn or realize that reneging is profitable. Because of backward unravelling, a decline of transfers within each OLG game might then be the result. Such a decline might become stronger with more experience. In discussing the results we will investigate whether such a pattern is visible in the data.

Finally, any design that allows for the possibility of learning or getting experience, simultaneously allows for the possibility that reputational considerations enter the picture. Usually, there is little hope of disentangling these two

[6] And, we believe this is for good reasons. But, as Roth (1995a, p. 80) notes, ''this is a question that could in principle arise when the predictions of a theory that assumes utility maximization are supported by the observed behaviour''.

effects. [7] We will not pay too much attention to this matter since the purpose of our paper is to study the effect of monitoring as it reveals itself in a comparison between treatment I and treatment N.

3. Design

Eleven experimental sessions, based on the model described above, were conducted on January 9, 10 and 11, 1995. Five sessions employed information treatment N (no information) and six sessions employed information treatment I (information). Students were recruited from Tilburg University with the announcement that the experiment would last for about an hour and that they would earn anywhere between 7 and 50 Dutch Guilders (i.e., between $4 and $29). No subject participated more than once, and most of them had no experience with economic experiments. Eight subjects participated in a session.

Upon arrival, subjects were randomly seated behind computer terminals, which were separated by partitions. Instructions were distributed and read aloud (see Appendix A). Then subjects were given several minutes to study the instructions more carefully and ask questions. One practice round was run before the 15 rounds that determined subjects' earnings. Then, an anonymous questionnaire asked for some background information (gender, age, major, motivation). Finally, subjects were privately paid their earnings in cash.

In each session the same pay-off function was used (see Eq. (3)). Each round consisted of a sequence of eight periods (0–7). Period 0 is an auxiliary period in which the first 'old' player was randomly selected from the eight participants. As no decisions are made in period 0, it will not enter the analysis. In each subsequent period (1–7), one of the remaining subjects was randomly selected to be the young player in that period. [8] The young player had to type a number T from the set $\{0, 1, \ldots, 7\}$, which determined his transfer to the old player. First-period consumption of the young player then was $C_1 = 9 - T$. Second-period consumption of the old player was $C_2 = 1 + T$. The old player was informed about the transfer received and her pay-off (in points) in the round: $U = C_1 \times C_2$. The young player became old in the next period and a new young player, randomly chosen from the remaining players, had to make a transfer decision. This procedure was repeated until period 7. Then all players had participated in the round, and a new round was started. After the last round, the points earned in the 15 rounds were added and

[7] Some clues about the relative effects of experience and reputation might be discernible in the data, however. For example, learning curves are typically steep in the beginning and become flatter with more experience. Effects of learning would then be strongest in the early rounds of the experiment. Effects of reputation, on the other hand, would reveal themselves most strongly in the last rounds of the experiment, as subjects would then be observed to 'cash in' on their reputation.

[8] Of course, in the experiment we did not use terms like 'young' and 'old' generation, but referred to these as Decider and Receiver, respectively.

converted into money at a rate of 1 point = 5 cents. In addition, each player received a lump sum (participation) payment of *f* 5. All aspects of the procedure were common knowledge.

The two information treatments differed as follows. In treatment N (no information), a player, when selected to enter the round and make a decision, was only informed about the period number $t \in (1, \ldots, 7)$. In treatment I (with information), a player was also informed about the transfer decisions made by the players in the previous periods of the round $(T_1, \ldots, T_{t-1})$. Note that in both treatments, a player was informed about the transfer made to him and his pay-off (in points) for the round when he left the round.

Some additional remarks have to be made with respect to the procedure. First, recall that the player, selected to be old in period 0 of a round, did not play the role of the young generation in that round. Her first-period consumption (when young) was then fixed at $C_1 = 2$. Similarly, the player, selected to be young in the last (7th) period of a round, did not play the role of the old generation of that round. His second-period consumption was then determined as $C_2 = 1 + T'$, where T' is the average transfer received by all previous old players in the round (rounded up). All of this was common knowledge.

Second, to facilitate computation, a pay-off table was included in the instructions. Third, in each period (1–6) a player was also asked to type his expectation $(0, \ldots, 7)$ regarding the transfer to be received from the player in the next period. Subjects were not paid to make (accurate) predictions. Therefore, we do not intend to make extensive use of these predictions in the analysis.

Finally, as mentioned earlier, the complexity of the OLG game requires the possibility for familiarization and learning by the subjects. In the (sparse) literature on OLG experiments, basically two designs can be distinguished. Lim et al. (1994) and Marimon and Sunder (1993) use a design that consists of one single, long OLG game in which subjects enter several times. Here, one could speak of a 'single OLG game with reincarnation'. Cadsby and Frank (1990) use a design that consists of a repetition of shorter OLG games, where in each game subjects enter only once. Hence, one can speak of 'repeated OLG games without reincarnation'. The advantage of the former design is that an OLG sequence has to be started and stopped only once. However, with one long OLG game, the effects of monitoring across periods, which is our prime interest, and learning over time become intertwined. Discerning the two effects might be served by separating the development of transfers over periods (monitoring) from the development over repetitions (learning). Therefore, in our basic design we opted for a repeated OLG game without reincarnation. [9] To check for the robustness of the results, however, we

[9] Two differences with the design of Cadsby and Frank are (a) that in our experimental setting eight, instead of two, successive generations participated in each OLG game, and (b) that the order in which the subjects participated in each OLG game was random in our design but fixed in Cadsby and Frank's.

have also run a series of five experimental sessions with one long OLG game with reincarnation (see Section 5).

4. Results

First, we will present the results regarding the level, development and stability of the transfers for both treatments (Q1). Then we will look more closely at the relation between present transfers and previous transfers (Q2). Finally, we discuss results regarding end-effects and experience, and we have a look at the individual data.

4.1. Development and variability of transfer levels

Recall that we have five (six) sessions with treatment N (I), and that in each session we have 15 repetitions of an OLG game consisting of 8 periods-generations with 7 transfer decisions. The overall average transfer, that is averaged over sessions, rounds and periods (1–7), is 1.90 in treatment N (no information) and 1.83 in treatment I (information). So, at the aggregate level, hardly any difference between the information treatments is visible. In fact, the transfer level is somewhat lower in treatment I, but the difference is not significant ($p = 0.93$, with a two-tailed Mann – Whitney U test with session averages as observations, $n_N = 5$, $n_I = 6$).

The average level of transfers of about 2 might seem low compared with the efficient stationary level of $T = 4$, but in terms of pay-offs the level of efficiency is quite high. A stationary level of transfers of $T = 4$ leads to a pay-off (in periods 1–7) of 25 points. A level of transfers of $T = 0$ leads to a pay-off of 9 points. The actual overall average pay-off (in periods 1–7) is 20.5 in treatment N and 20 in treatment I. Hence, a stationary transfer level of $T = 4$ leads to an efficiency gain of 16 (= 25–9) points, of which about 11 (= 20–9) are actually realized in the experiment. In other words, the voluntary transfers observed in the experiment achieve an overall efficiency level of almost 70%.

Fig. 1 presents the level of transfers for each round of the OLG game (averaged over periods and sessions). For both treatments a two sigma (standard deviation) range for the average transfer has been added. It can be seen that the development of the average level of transfers over the rounds hardly differs between the two treatments. Furthermore, for both treatments, the average transfers seem to decrease in the early rounds (1–5), remain almost constant during the middle rounds (6–10), and decrease again in the final rounds (11–15). The average transfer levels in these three subsets of rounds are 2.18, 1.95 and 1.57 for treatment N, and 2.06, 1.90 and 1.53 for treatment I. The decline over time is statistically significant. That is, for both treatments the average transfers in rounds 1–5 are significantly larger than in rounds 11–15 (Wilcoxon matched-pairs signed-rank

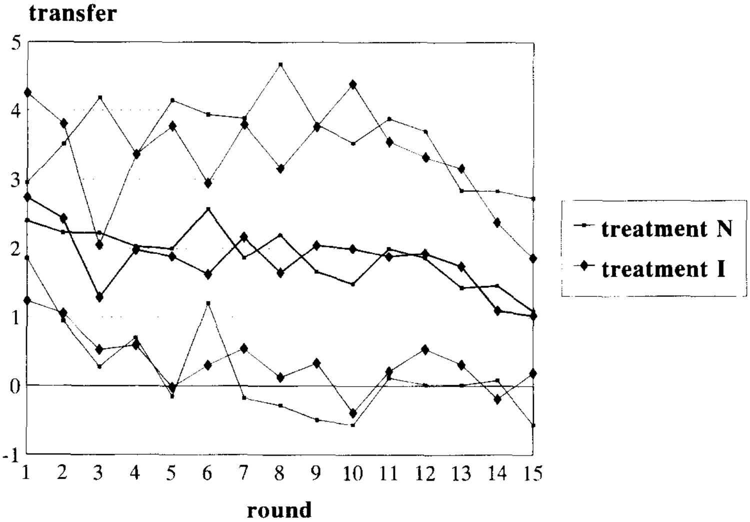

Fig. 1. Average transfer by round by treatment plus a two sigma dispersion range.

tests with session averages as observations). However, for both treatments the average transfers in rounds 6–10 are not significantly different from those in rounds 1–5 and rounds 11–15. Differences between the two treatments are not significant in any subset of rounds.

The sigma ranges provide information about the stability of the voluntary transfer system. As argued in Section 2, the possibility of monitoring previous generations might affect the stability of the transfers, be it at a high or at a low level. If, in treatment I, the early generations start with a low (high) level of transfers, then this low (high) level might be followed by subsequent generations. No such following pattern is possible in treatment N. However, Fig. 1 shows that no systematic difference in the variability exists between the two treatments. Furthermore, the dispersion is rather constant over time.

Fig. 2 presents a histogram of the distribution of the transfers in both treatments. In both treatments, the modal transfer is zero and transfers of 1 to 4 each occur at a rate of about 0.15. Transfers larger than 4 are rare. Although there are some differences between the two treatments, the distributions are very similar.

In summary, no difference in the level, development, distribution or stability of the transfers between the two information treatments is detectable in the data. The answer to question Q1, whether information facilitates the occurrence of positive transfers, is clearly negative. Nevertheless, the average transfer level, of about 50% of the collectively efficient level, is rather high. Even in later rounds, the average transfer is clearly bounded away from zero.

It might be noted here that our results resemble those of public-good experiments. In those experiments voluntary contributions to public goods are also

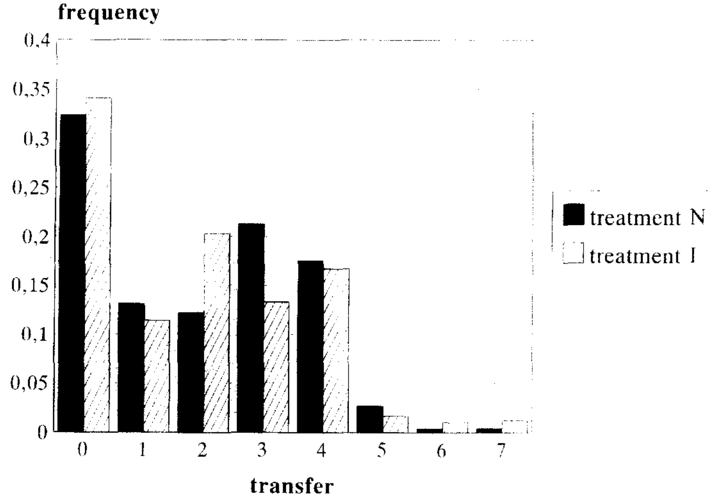

Fig. 2. Histogram of the transfers in treatments N and I.

typically bounded away from zero. This has led to a number of explanations that attribute this result to the design of the experiment rather than the motivations of the subjects. For example, the typical design, like ours, has the non-cooperative equilibrium outcome at the lower bound of the action space. As a consequence, any errors would result in positive contributions. Keser (1996), however, uses a design in which the non-cooperative outcome is in the interior of the action space. Her results indicate that the vast majority of the contributions are above the non-cooperative outcome and not symmetric around it. This casts serious doubt on the error theory of contributions. Also, it has been argued that 10 to 15 repetitions is perhaps not enough for subjects to learn and play the non-cooperative equilibrium. However, Isaac et al. (1994) find that contributions to public goods do not approach zero even with 40 to 60 repetitions. They suggest that the decay of the contributions is inversely related to the number of repetitions. Hence, increasing the number of repetitions is not likely to lead to zero contributions (or transfers). [10]

4.2. A close look at the relationship between present and previous transfers

The results of the previous section suggest that we will not find strong indications of monitoring across generations. The present section confirms this suggestion.

[10] Also, it has been argued that subjects contribute to public goods because the experimenter expects them to do so. To test whether such 'moral suasion' (Ledyard, 1995, p. 169) is an important factor, it would be interesting to use a 'double-blind' procedure, like Hoffman et al. (1994) do in a bargaining experiment. We do not know of any public-goods experiments that try a double-blind procedure.

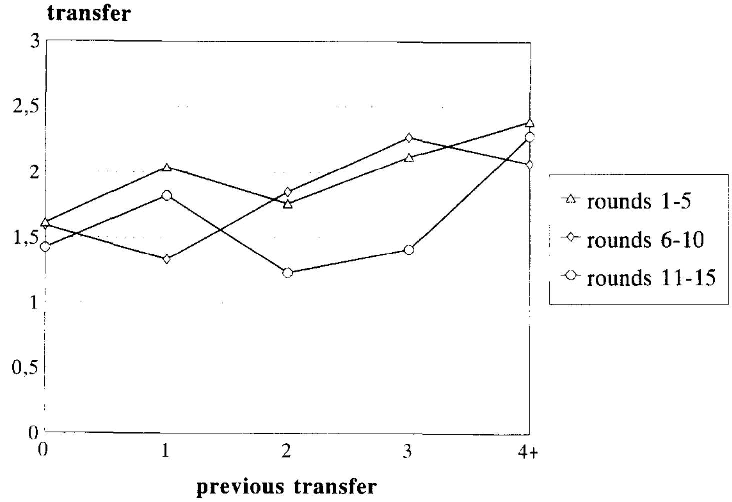

Fig. 3. Transfer conditioned upon the previous transfer in the same round in treatment I.

For treatment I, Fig. 3 displays the average levels of T_t as a function of the transfer of the previous generation T_{t-1}, $t \geq 2$. Values of T_{t-1} larger than four are rare. Therefore, these are pooled with values of four. Furthermore, the results are displayed separately for rounds 1–5, rounds 6–10 and rounds 11–15 in order to control for the decline of the average transfer level over the rounds. Fig. 3 suggests a weak positive relation between the transfers of the previous and the present generation. For instance, the average transfer level after $T_{t-1} = 4+$ is about 50% higher than the transfer level after $T_{t-1} = 0$. However, the difference is insignificant at $p = 0.46$ for rounds 1–5, $p = 0.69$ for rounds 6–10 and at $p = 0.14$ for rounds 11–15 (two-tailed Wilcoxon matched-pairs signed-rank test with the 6 session averages as observations). In addition, no monotonic positive relation between T_{t-1} and T_t is observed. For example, the average transfer after $T_{t-1} = 2$ is lower than after $T_{t-1} = 1$ in rounds 1–5 and round 11–15. Hence, we do not find strong signs for the importance of monitoring, rewarding and punishing. [11]

This conclusion can even be strengthened if we plot a similar figure for treatment N (Fig. 4). Note that in treatment N players could not consciously establish a relationship between T_{t-1} and T_t because T_{t-1} was not known to a subject when (s)he decided on T_t. Yet, as a comparison with treatment I it is

[11] In treatment I, player P_t can reward or punish player P_{t-1} for how this player has treated player P_{t-2}. The final player, P_7, might have a somewhat stronger motivation to reward or punish player P_6 ('real gift exchange') as the transfer of P_6 not only affects P_5, but also P_7 himself (because P_7 receives the average of all previous transfers). However, we do not find any sign for this. The relation between T_t and T_{t-1} is not stronger for P_7 than for the earlier players, P_1 to P_6.

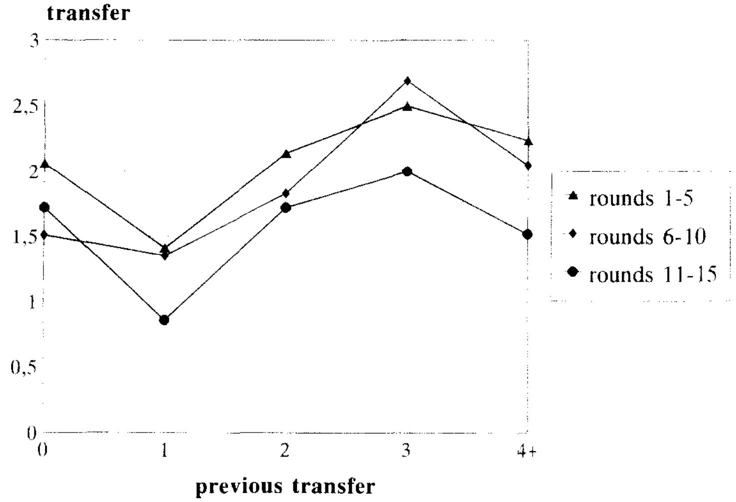

Fig. 4. Transfer conditioned upon the previous transfer in the same round in treatment N.

interesting to display this relationship for treatment N, too. Fig. 4 suggests a weak positive relation between present and previous transfer for treatment N, which is of similar magnitude as the one for treatment I. For treatment N, this weak positive relationship between T_{t-1} and T_t can be explained by the gradual decline of the transfers across rounds. Since this decline also plays a role in treatment I, it casts doubt on the importance of monitoring as an explanation for the (already weak) positive relation observed in Fig. 3.

Figs. 3 and 4 suggest, moreover, that the relationship between T_{t-1} and T_t does not change over time. A more detailed picture of the development of this relation over time can be obtained if we plot the correlation coefficient between T_{t-1} and T_t in each round. Fig. 5 displays the development of the correlation coefficients for both treatments. The figure shows that the correlation coefficient is about equal in both treatments and that there is no strong, systematic change across the rounds. This picture can be confirmed statistically using Mann–Whitney and Wilcoxon tests for subsets of rounds, respectively. Hence, there are weak signs of punishing and rewarding, but the effect is not strongly 'intrinsic' (it is weak in the early rounds), nor is it 'learned' during the experiment (it remains weak in the final rounds).

One might argue that a monotonic relation between T_{t-1} and T_t is not what we should be looking for. If subjects are more strategically motivated, we should expect to observe 'trigger-like strategies'. As argued in Section 2, trigger strategies suggest that present transfers must be conditioned on the transfers of *all* previous generations. No version of trigger-like strategies is visible in the data, however. The average transfer in case there should be a punishment is not distinguishable from the transfer in case there should be no punishment.

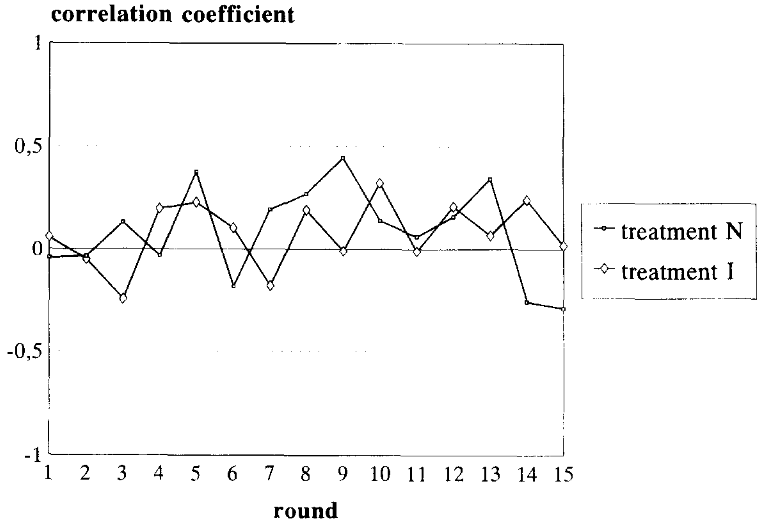

Fig. 5. The correlation coefficient between the transfer and the previous transfer by treatment.

We also tested the confidence-building approach discussed in Section 2 (Eq. (5)). A result that is in line with this approach is that, on average, transfers levels T_t are lower in case the previous player has decreased the transfer level ($T_{t-1} < T_{t-2}$), than in case she has not decreased the transfer level ($T_{t-1} \geq T_{t-2}$). The average transfer in the former case (1.68) is significantly lower than the average transfer in the latter case (1.81). The effect, though significant, is small and shows no strong development over time.

In summary, as could be expected in view of the negative answer to question Q1 in the previous section, only a moderate relation between present and previous transfer levels is found in the data of treatment I (and of treatment N). Moreover, this relation seems to become neither stronger nor weaker with more experience. [12]

[12] In the experiment we also asked subjects' expectations about the transfer of the next generation (see Section 3). Two relationships could be interesting here. Firstly, the relationship between one's own transfer and the expected transfer (as a measure for anticipated rewarding or punishing), and, secondly, the relationship between the expected transfer and the actually received transfer (as a measure for the accuracy of the expectation). Overall, it appears that the first (cor)relation is stronger for treatment N (0.57) than for treatment I (0.41). Hence, rewards and punishments are anticipated more strongly if, by construction, they cannot be provided. What is more interesting, perhaps, is that this (cor)relation shows no systematic development over the rounds, that is, it becomes neither stronger nor weaker. The second relation is very weak (a correlation coefficient of about 0.10) in both treatments. Hence, subjects seem to be bad predictors. Again, the correlation shows no systematic development over the rounds. As subjects were not rewarded for making (accurate) predictions we do not wish to put much weight on these results, however.

4.3. End effects, experience and individual data

As argued before, in addition to the transfers of previous generations in the round, several other factors may affect the level of transfers and its development over time. In this subsection we briefly analyze end effects, experience, and look at individual data.

First, our design consists of a repetition of finite OLG games. Subjects who enter an OLG game in the last period, know that they are last in the sequence of that round. Hence, backward-induction reasoning might induce them to renege on any (implicitly agreed upon) positive transfer level by previous generations. Moreover, due to the backward induction unravelling argument, we might expect to observe a gradual decline of the transfer level over the periods in each round of the OLG game.

A weak final period end effect is indeed visible in the data. For both treatments, Fig. 6 presents the level of transfers for each period of the OLG game (averaged over rounds) and the dispersion, given by a two sigma range. The average transfer level in period 7 is somewhat smaller than the average level over periods 1–6. When considering both treatments together, the difference is marginally significant (Wilcoxon matched-pairs signed-rank tests, $p < 0.10$), but it is not significant when each treatment is taken separately. Furthermore, no monotonic development over the periods of the OLG game exists. On average, earlier and later generations

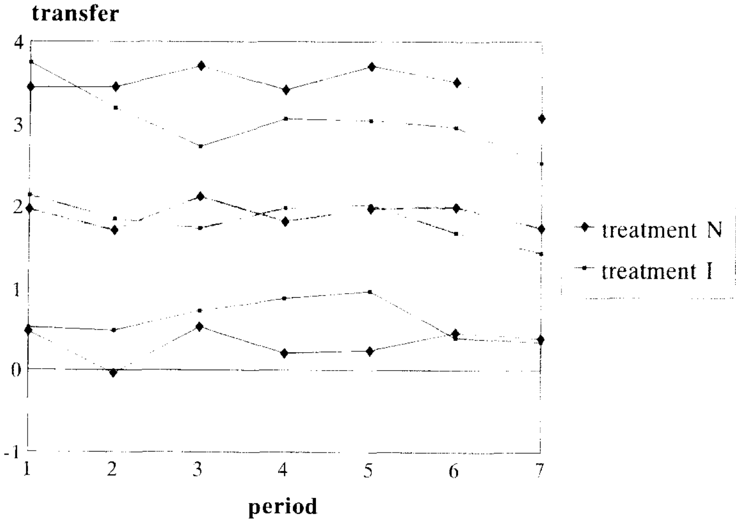

Fig. 6. Average transfer by period by treatment plus a two sigma dispersion range.

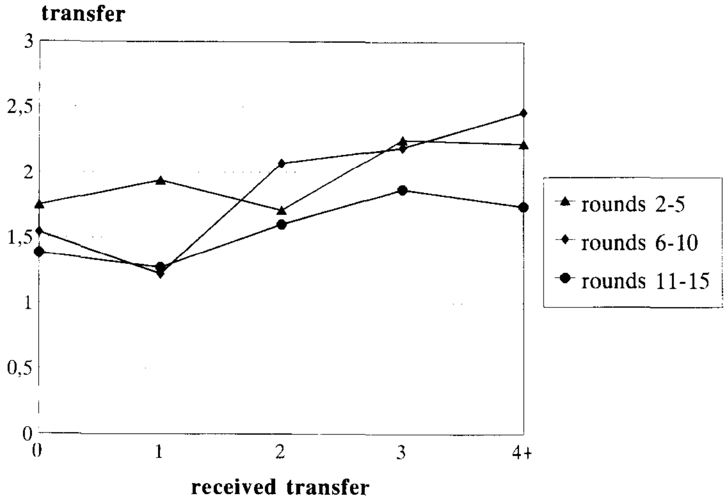

Fig. 7. Average transfer as a function of the transfer received in the previous round (both treatments).

in the OLG game transfer a comparable amount of their endowment. [13] Moreover, the variability of the transfers does not seem to depend on the number of the period.

Second, it is to be expected that subjects adapt their behaviour in response to their experience in the previous round(s) of the experiment. A representative picture of the size of the effect of experience is given in Fig. 7, which combines both treatments. Average transfer levels in round R $(=2,\dots,15)$ are related to the transfer level received in the previous round $(R-1)$. The figure shows that, on average, subjects tend to give a somewhat higher transfer if they have been 'treated well' in the previous round. To control for the gradual decline of the transfer level over rounds (see Fig. 1) the effect is displayed separately for early (2–5), middle (6–10) and later rounds (11–15). This separation shows that the effect is not substantially different in earlier or later rounds. A weak positive relation can be observed, but it becomes neither stronger nor weaker over time. [14]

Until now we have mainly focused on aggregated data. A look at the individual data, however, reveals that some 28 subjects followed an almost constant strategy. That is, in each round, they chose about the same transfer level, irrespective of period, round or history. Twelve of these players could be characterized as 'strict

[13] No clear development across rounds can be observed. The end effect becomes neither stronger nor weaker with experience.

[14] Comparing the average transfer after having received 0 in the previous round, with the average transfer after having received 4, gives no significant difference for rounds 2–5 or rounds 11–15, when taking the two treatments together (Wilcoxon matched-pairs signed-rank test with 11 observations). Taking the treatments separately, gives a significant effect only for treatment N in rounds 2–5.

gamesmen' (6 in treatment I and 6 in treatment N). They chose a transfer of zero in each round, with at most three exceptions. Ten subjects could be labelled 'altruists' (4 in treatment I and 6 in treatment N). They transferred at least three in each round, with at most three exceptions. Another 6 subjects chose an almost constant transfer level in between 0 and 4. The presence of these 28 subjects with an almost constant transfer, could blur the (quantitative) effects of monitoring, end-effects, or learning. Therefore, we have repeated all previous analyses for the subset of 60 (= 88–28) players with a non-constant strategy. The overall conclusions, however, hardly change. Some of the above effects become quantitatively stronger, but the differences with the full-group analysis are remarkably small.

Besides the 28 subjects who followed an almost constant strategy of giving, we (only) found 13 subjects who were strongly and systematically affected by the immediate history of play. In particular, we found 7 subjects who clearly followed the transfer of the previous player in the round. These 'followers' are characterized by a high and significant positive correlation between their own transfer and the transfer of the previous player in the round (all these subjects are in treatment I). In addition, 6 subjects could be identified who reacted strongly on the transfer received in the previous round. They are characterized by a high and significant positive correlation between their present transfer and the transfer received in the previous round. For example, two subjects consistently chose a transfer that was somewhat lower than the one received in the previous round. Apart from these 13 subjects, we were unable to find other subjects who seemed to react to the previous transfer in any consistent or systematic manner. Hence, this analysis of the individual data more or less confirms the previous aggregate data analysis. There are weak signs for a positive relation between present and previous transfers (for some subjects), but surely this is not a main factor for explaining the observed transfer levels.

Finally, it would seem useful to have some overall picture of the relative effects of the various factors, analyzed separately above. The simplest way to give such a picture is to regress transfer levels on these multiple factors. In particular, we ran OLS-regressions of the following behavioural equation: [15]

$$T_t^R = \alpha_0 + \alpha_1 R + \alpha_2 T_{t-1}^R + \alpha_3 \left(T_{t-1}^R - T_{t-2}^R\right) + \alpha_4 t + \alpha_5 R^{R-1} + \alpha_6 T^{R-1} + \eta. \tag{6}$$

The equation reads as follows. A subject's transfer T_t^R in period t (= 1, ..., 7) is hypothesized to depend on a constant (α_0), the round number (R), the transfer of the previous generation in the round (T_{t-1}^R), the difference of the transfers of the

[15] Implicitly, the regression assumes that observations are independent, which is not the case, strictly speaking. Regressions using individual decisions are rather common in experimental research, though, particularly for 'illustrative' purposes.

Table 1
Regression of the behavioural equations for the two treatments

	Treatment N		Treatment I	
	Value	p-value	Value	p-value
α_0	0.92	0.01	1.17	0.00
R	−0.01	0.47	−0.01	0.68
T_{t-1}^R			0.15	0.02
$T_{t-1}^R - T_{t-2}^R$			−0.06	0.19
t	−0.05	0.32	−0.09	0.09
R^{R-1}	0.20	0.00	0.03	0.58
T^{R-1}	0.48	0.00	0.38	0.00
# obs.		350		420
$\bar{R}^2$		0.30		0.17

previous two generations in the round (T_{t-1}^R–T_{t-2}^R), the period number (t), the transfer received in the previous round (R^{R-1}), the transfer given in the previous round (T^{R-1}) and an error term (η). In sequence, these are the factors analyzed separately above.

Table 1 presents the regression results, for each treatment separately. The exogenous variables are in the first column. Parameter estimates and corresponding significance levels of the t-statistic are in the next two columns. [16] In treatment N, T_{t-1}^R and $T_{t-1}^R - T_{t-2}^R$ have not been included, as this information was not available to the subjects. The monitoring possibility appears to result in some actions on the basis of the transfer by the previous player: in treatment I, T_{t-1}^R has a significantly positive effect, although its quantitative effect is small. On the other hand, we do not find any indication for the existence of a confidence parameter as suggested by Eq. (5); the estimated coefficient of variable $T_{t-1}^R - T_{t-2}^R$ is insignificant. In treatment N, owing to the lack of information, decisions are mainly based on the transfer received in the previous round (R^{R-1}) and the transfer given in the previous round (T^{R-1}), the latter indicating some presence of personal inertia or idiosyncrasy. The latter variable has also a significant effect under treatment I, whereas the former has no effect.

Concluding, the regression results show some small impact of monitoring and some presence of rewards and punishments. However, in line with our findings in the foregoing analysis, the effects seem to be very small. On average, subjects seem to balance the trade-off between the collective efficiency of a transfer scheme and the individual temptation to defect on such a scheme somewhere 'halfway', with marginal adjustments in response to personal experience.

[16] Because of the regression specification, only the results in periods 3 to 7 of rounds 2 to 15 can be used.

5. Two additional designs

To check on the robustness of the results, we ran two additional sets of experiments with information treatment I (in March 1995, with eight inexperienced subjects in each of the nine sessions). As these additional experiments confirm the picture presented in the previous section, we do not dwell on them extensively.

First, the results described above might be sensitive to our choice for a 'repeated OLG game without reincarnation'. Therefore, we had five additional experimental sessions with 'one long OLG game with reincarnation'. These experimental sessions consisted of one OLG game of 120 ($= 15 \times 8$) periods. In this design, the OLG sequence has to be started and finished only once. The young player entering in each period was determined randomly with two restrictions (about which the subjects were informed). First, each subject would enter the game fifteen times, and, second, a player who entered in a particular period could not enter in the next two periods. Furthermore, in line with information treatment I, a player entering a period was informed about the transfer levels in the preceding eight periods.

The results show that the average transfer level in this design (1.38) is lower than in our basic design (1.83). The difference, however, is not significant (Mann–Whitney U test with session averages as observations). Also in other respects the results are similar. For example, in each session the transfer level is relatively volatile, there is a slow decline with experience, and there are only weak signs of rewarding and punishing.

A second worry we had regarding our design, was that the strategy space was 'too large' to employ trigger-like strategies. The more options a player has, the more difficult it will be to coordinate on any implicit social contract. For example, subjects might not agree on the transfer level aimed at by a contract. Similarly, it might be unclear whether transfer levels of, say, 2 should also be punished or that punishment should only occur with levels of 0 and 1. To simplify the development and employment of trigger strategies in support of a social contract, we carried out four additional sessions (treatment I) with transfer levels restricted to the set $\{0, 4\}$ instead of $\{0, 1, \ldots, 7\}$. This design, in a sense, solves the coordination problem for the subjects. If an implicit contract aims at a positive transfer level, it is clear what this level should be. Moreover, defining defection is much easier.

It turns out that the restriction of the strategy space results in lower average transfer levels (1.04) than in treatment I of the basic design (1.83). The difference is significant at the 5% level (two-tailed U test with session averages as observations). Moreover, there is no indication of any increased success or even an attempt to use trigger-like strategies. Under trigger strategies, the least we should expect is that the average transfer in case there should be a punishment is smaller than the transfer in case there should be no punishment. However, we do not find a significant difference. On the contrary, the weakly positive relation between T_t

and T_{t-1} displayed in Fig. 3, becomes even weaker. Average transfers in response to $T_{t-1} = 0$ (1.04) are almost identical to average transfers in response to $T_{t-1} = 4$ (1.02). What the restriction of the transfer set seems to do, is to make it more difficult for subjects to balance the trade-off between individual and collective rationality. On average, subjects seem to make a balance at a value of about 2, but restricting the choice to 0 or 4, seems to tip the balance downward.

In summary, the additional series of experiments confirms the general picture of our baseline design. Although average transfer levels are somewhat lower, they are still clearly bounded away from zero. More importantly, the absence of strong signs of rewards and punishments or trigger strategies in our baseline design is not due to 'repetition of an OLG game without reincarnation' or due to the 'large strategy set'.

6. Conclusion

We started our paper with the observation that, in spite of the financial strain, the support for public pension systems does not seem to decrease strongly. This raises the question of the basic motivation and the driving force behind the establishment and maintenance of intergenerational transfer systems. Theoretical explanations suggested that there is a strong positive link between the decisions of past, present and future generations. The incentive of the present generation to renege, can be mitigated by the possibility of the next generation to monitor the adherence to the scheme and to reward or punish accordingly.

The main result of our experiments is that the level and stability of the transfer system is not furthered by the possibility of monitoring transfers of past generations. Furthermore, only weak signs of rewards and punishments or trigger-like strategies are found. There appears to be almost no (cor)relation between the transfers of present and past generations. Notions like reward and punishment hardly seem to play a role. Yet, substantial levels of voluntary transfers can be observed in the experiment. Subjects seem responsive to the trade-off between the collective interest of a transfer scheme and the individual temptation to defect on such a scheme. On average, subjects seem to balance the trade-off somewhere about 'half-way', with marginal adjustments in response to personal experience.

It is interesting to relate these results to the typical results of both bargaining experiments and public-goods experiments. Although public-goods experiments lack the intertemporal structure that characterizes our experiment, there also, the typical result is that subjects balance the trade-off between individual and collective rationality somewhere halfway. With repetition and experience, contribution rates fall, but usually they stay bounded away from zero. In bargaining experiments, a general finding is that the power of the receiving side is a prime determinant of the 'generosity' of the proposing side. In bilateral relationships, the possibility of monitoring and reciprocating is an important check on the power of the proposer. Our results, on the other hand, do not show a strong impact of monitoring.

This would suggest that in the OLG experiment the public-good feature may be more important than the intertemporal bargaining feature. It would seem that the interdependence in the underlying OLG structure creates a kind of social cohesion which is independent of the possibility of monitoring. The average individual appears to have a generic, though moderate willingness to support transfers, which is almost irrespective of the past behaviour of the recipient. Notions like monitoring, rewarding and punishing hardly seem to play a role. If a norm is at work, it must be one which does not necessarily depend on a monitoring possibility. One such norm that is more in line with our results is the notion of (anticipated) reciprocity, advanced by Sugden (1984) to explain voluntary contributions to public goods. Contrary to the more traditional notion of reciprocity as a kind of *quid pro quo* (Gouldner, 1960), Sugden's norm of reciprocity entails that you contribute to a collective good to the extent that you anticipate others to contribute (not to the extent that you observe others to contribute). Adherence to this norm of anticipated reciprocity does not presuppose a monitoring possibility. In this sense, it is in line with our finding that monitoring does not effect the level of voluntary transfers.

A natural question to ask is to what extent the results of the experiment are likely to generalize to the wider world. A good reason to maintain some healthy scepticism is that we do not know to what extent the motivations and attitudes of the subjects in the experiment correspond to those of the individuals in the field of substantive interest. Consequently, some of the phenomena that we observe in the laboratory may have diminished importance in the field, and phenomena that have no opportunity to emerge in the laboratory may assume more importance (Roth, 1995b, p. 329). For example, it is possible that we observe voluntary transfers in the lab due to a norm (e.g., anticipated reciprocity), that plays no role in the field. On the other hand, the popular support for pension schemes in the field may rest on an attitude (e.g., intergenerational altruism) that we did not induce on the laboratory subjects. Therefore, it is necessary to "generalize with caution" (Holt, 1995, p. 424). Generalizibility (sometimes called external validity or parallelism) is not an a self-evident axiom or a matter that can be resolved theoretically. Ultimately, of course, it is an empirical issue. [17]

Of some interest in this respect, are the results of a large-scale survey in the Netherlands (Van der Heijden et al., 1995), which indicate that people in general have a very poor knowledge of the (unfavourable) relation between the present

[17] Fortunately, there is some cause for optimism in this respect. For example, in a clever experiment, Erev et al. (1993) successfully generalized their laboratory results (on the effects of intergroup competition on free riding) to a field setting. Furthermore, many results obtained with the typical student subjects, have been replicated with populations of more substantive interest (see, e.g., DeJong et al., 1988; Mestelman and Feeny, 1988; Dyer et al., 1989).

costs and future benefits of the current public pension scheme. In spite of that (or, perhaps, because of that) the same respondents expressed a strong willingness to support the current pension scheme. This result is in line with our main experimental result that the support for a transfer scheme does not depend much on monitoring. In a loose sense, one could say that in both the experiment and the survey, the private insurance element of transfers ('bargaining frame') seems less important than the collective public-good element in the transfer system.

Acknowledgements

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Appendix A. Instructions

A.1. Introduction (read aloud only)

You are about to participate in an experimental study of decision-making. The experiment will last for about one hour. The instructions of the experiment are simple and if you follow them carefully and make good decisions you may earn a considerable amount of money. All the money you earn will be yours to keep and will be paid to you, privately and confidentially, in cash right after the end of the experiment

{For the experiment it is of crucial importance to have 8 participants. However, experience shows that often 1 or 2 persons do not show up or do not show up in time. Therefore, we need to have 10 instead of 8 subscriptions. This sometimes has, as now, the consequence that too many participants are present and that 1 or 2 persons cannot participate in this experiment. These persons can still put their name down for one of the following experiments and receive *f* 10 for any inconvenience. These persons are determined by lot because one or two blank envelopes are added to the box with seating numbers, unless one of you checks in voluntarily not to participate in the experiment and receive *f* 10 instead.}

Before we go on with the instructions, I would like to ask all of you to draw an envelope from this box and open it. The number denotes the terminal you have to be seated. {If you draw a blank envelope you cannot participate in the experiment and you receive *f* 10.}

We will distribute the instructions of the experiment now and read through them together. After that, you will have the opportunity to ask questions. From now on, you are requested not to talk to, or communicate with, any other participant.

Instructions (distributed and read aloud) [18]

A.2. Decisions and earnings

The experiment exists of fifteen separate *rounds*. In every round, each of you will earn a certain amount of *points*. At the end of the experiment the points earned in the 15 rounds are added up for each participant separately. Every point earned is worth 5 *cent* ($\approx$ \$0.028) at the end of the experiment. In addition to this, all participants receive a fixed extra amount of *f* 5. Your total earnings will thus be equal to *f* 5 plus the number of points earned times 5 cent. Now, we describe how the points earned in each round will be determined.

Each round will consist of seven *periods*. In every period two participants are involved, a so-called *Decider* and a *Receiver*. In each round of seven periods, every participant will, in principle, once have the role of Decider and once the role of Receiver. The earnings of a participant in a round are determined by the final assets of a participant in the period in which he or she is a Decider, and by the final assets of the participant in the period in which he or she is a Receiver. We denote the final assets as Receiver by E_O and the final assets as Decider by E_B. The earnings in points of a participant in a round are determined by the product of the final assets as Receiver and the final assets as Decider. The earnings of a participant in a round are thus equal to $E_B \times E_O$ points. Next, we describe how the final assets as Decider E_B and the final assets as Receiver E_O are determined.

In the first period of a particular round, two participants are randomly assigned by the computer to be Receiver and Decider. The Receiver starts with an endowment of 1, whereas the Decider starts with and endowment of 9. The Decider has to decide which part of his or her endowment that he or she wants to transfer to the Receiver. This transfer, which we will denote by T, is 0 at the minimum, and 7 at the maximum. After the Decider has decided about the transfer T to the Receiver, the final assets of the Receiver are $E_O = 1 + T$, and those of the Decider are $E_B = 9 - T$. After the Decider has decided about her or his transfer to the Receiver, the next period of the round will be started. The participant who was the Receiver in the previous round is finished for this round.

In the next period, the Decider of the previous period will now be the Receiver. The new Decider is selected by the computer from the participants who have not yet taken turns in this round. The determination of the final assets of the new

[18] The text between square brackets ([ ]) was added in treatment I. The text between braces ({ }) was added when more than 8 participants showed up.

Receiver and Decider in this period is similar to the previous period. The Receiver starts with an endowment of 1 and the Decider starts with an endowment of 9. The Decider decides again about the part of her or his endowment that will be transferred to the Receiver. This transfer T determines the final assets of both participants: $E_O = 1 + T$ for the Receiver and $E_B = 9 - T$ for the Decider.

Subsequently, a new period will be started in which the old Decider becomes the new Receiver and the new Decider is selected from the participants who have not yet taken turns. In this way, we continue up to and including the seventh period. After that, the next round of seven periods will be started.

Note that the participant who is Receiver in the first period of a round will not take a turn as Decider in that particular round. For this participant the final assets as Decider are determined to be $E_B = 2$. Further, the participant who is Decider in the seventh period of a round will not take a turn as Receiver in that round. For this participant the final assets as Receiver E_O will be equal to the average final assets of all seven Receivers in that particular round, so including the current Receiver.

As said, your earnings in a round are determined by the product of your final assets E_B in your role of Decider and the final assets E_O in your role of Receiver. Your assets E_B are dependent on your transfer to the Receiver in the period you are Decider and your assets E_O are dependent on the transfer from the Decider to you in the period you are Receiver. To facilitate the determination of your earnings, you may use the table below.

The table states your earnings in points in a round dependent on the transfer *from you* to the Receiver when you are Decider and the transfer *to you* by the Decider when you are Receiver. In this table the rows present the transfer from you as Decider to the Receiver and the columns present the transfer to you as Receiver from the Decider. When you first look for the transfer *from you* in the row and then go to the right to the column stating the transfer *to you*, you can read your earnings in points, $E_B \times E_O$, for the round. The earnings in money are determined by multiplying the stated amount in points by 5 cents.

		Transfer *to you* from the Decider when you are Receiver							
		0	1	2	3	4	5	6	7
Transfer *from you* to the Receiver when you are Decider	0	9	18	27	36	45	54	63	72
	1	8	16	24	32	40	48	56	64
	2	7	14	21	28	35	42	49	56
	3	6	12	18	24	30	36	42	48
	4	5	10	15	20	25	30	35	40
	5	4	8	12	16	20	24	28	32
	6	3	6	9	12	15	18	21	24
	7	2	4	6	8	10	12	14	16

When you are the *first* Receiver in a round, your final assets as Decider are determined to be $E_B = 2$. In that case, your earnings in points, $E_B \times E_O$, only depend on the transfer from the Decider *to you* $E_O = 1 + T$. You can read these earnings from the table by looking for the column with the concerning transfer *to you* in the bottommost row (with transfer *from you* is 7).

When you are the *last* (seventh) Decider in a round, your final assets as Receiver E_O are determined as the average final assets of all seven Receivers in that round (rounded up). Your earnings in points, $E_B \times E_O$, are determined via the table by the row with the transfer *from you* and the column of which the number equals the average transfer to all Receivers in that round.

A.3. Procedure and usage of the computer

After we have gone through the instructions, first a practice round will be run. After the practice round, the fifteen rounds that determine your earnings for this experiment will be run.

In every round the computer, in a completely random manner, determines who will get the roles of Receiver and Decider in the first period. On the screen the Receiver will see the message "You are the first Receiver". The Decider will see the number of the current period on the upper left part of the screen. [Next to it, you will see "INFORMATION until now". In the first period this information will only consist of the message "There have been no previous periods in this round"]. Underneath, the Decider will see the question "How much of your endowment do you transfer (0–7)?" The Decider has to type an integer from 0 up to and including 7. The number typed is the transfer T to the current Receiver.

Next, the current Decider will be asked the question "How much do you expect to receive?". Here, the Decider types an integer from 0 up to and including 7, dependent on her or his expectation about the transfer she or he expects to receive as Receiver in the next period. This expectation is used by us when analyzing the experiment, but your earnings will be unaffected by it. Besides, the other participants are not informed about your expectations stated.

After the Decider has taken her or his Decision, the current Receiver will see the number of the present period on the screen and underneath how much she or he receives and her or his earnings for the round. After the Receiver has taken note of this, he or she has to press Return to close the current period and to start the new one.

The Decider of the previous period becomes Receiver in the new period and the computer will select a new Decider from the participants who have not yet taken turns in this round. This new Decider sees the number of the current period on the upper left part of the screen [and next to it "INFORMATION until now". Underneath, it is reported for every decider of the previous periods how much he or she has transferred, how much he or she has received as Receiver and what her or his earnings are for the round. For the Decider of the previous period it is only shown how much he or she has transferred because this Decider is Receiver in the current period] and underneath the question "How much of your endowment do you transfer (0–7)?" After this decision has been typed and passed on to the current Receiver a new period will be started in which the Decider of the previous period will be the new Receiver. This procedure will be repeated up to and including period 7.

In all periods, a new Decider is randomly selected by the computer from all participants who have not yet taken turns in that round. After all seven periods in a round have been completed, the first period of the next round is started. Then, a new Receiver and Decider are again randomly selected by the computer for the first period and time after time a new Decider for the next periods is selected. Therefore, the order in which the participants take turns in every round is not fixed but is determined time after time by the computer in a random way. You cannot know when it will be your turn in a round. Moreover, you cannot know to whom you will be paired in a certain period.

A.4. Summary

The experiment consists of 15 rounds, and every round consists of 7 periods. In every period, two participants are involved, a Receiver and a Decider. The endowment of the Receiver is 1 and the endowment of the Decider is 9. The final assets of Receiver and Decider are dependent on the transfer T of the Decider to the Receiver: $E_O = 1 + T$, $E_B = 9 - T$. In every round, in principle, you are the Decider in one period, and the Receiver in the next period. Your earnings in points in a round are determined by the product of your final assets in the period you are Decider and your final assets in the period you are Receiver: $E_B \times E_O$.

The participant who is Receiver in the first period will not act as a Decider in that round. His or her final assets as Decider are determined to be $E_B = 2$. The participant who is Decider in the last period will not act as a Receiver in that round. His or her finals assets as Receiver E_O are determined as the average final assets of all seven Receivers of that round, so including the current Receiver.

In every period, only the current Decider and Receiver are informed about the size of the transfer given from the Decider to the Receiver. [instead of the previous line in information condition 2: In every period the current Decider receives information about the transfer of the deciders in the previous periods.]

The order in which the participants participate in the periods of a certain round is determined by the computer in a completely random way time after time. You will never be able to know when it will be your turn in a round or to whom you will be paired in a certain period.

A.5. Final remarks

After the last round, you will first be requested to answer some questions to evaluate the experiment. This questionnaire is anonymous. We can link your answers to your seat number but not to your name. After that, you will be called by your seat number to receive your earnings privately and confidentially. Your earnings are your own business; you do not need to discuss with anyone. It is not allowed to talk to or communicate with other participants during the experiment in either way.

On your table you will find an empty sheet, which you can use to take notes. Additionally, you will find a sheet labelled 'REMARKS'. On this sheet you can make remarks about the instructions or your decisions.

You get a couple of minutes to go through the instructions and to ask questions. When you want to ask something, please raise your hand. One of us will come to your table to speak to you.

After that we will start the practice round.

Are there any questions?

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